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Interactive comment on "Widespread persistent polar stratospheric ice clouds in the Arctic" *by* Christiane Voigt et al.

Anonymous Referee #3

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The paper by Voigt et al. describes the occurrence of polar stratospheric ice clouds during the Arctic winter 2015/16 by means of CALIPSO and airborne lidar observations. The overall situation of this Arctic winter is described by use of CALIPSO observations and ECMWF model fields. Later in the paper one case study from an airborne lidar is presented and used to derive a new threshold for the classification of ice PSCs.

The paper is not very well structured and the main idea of the paper is not clear to me. Is the paper supposed to be an overview paper of the PSC situation of the winter 2015/16 or is it to introduce the new threshold? The paper requires major revisions to achieve either objective. The CALIPSO observation used to present the overall and unique situation of the winter 2015/16 seem to have been analysed with the old threshold. At least that's how I understand it from the structure of the paper. Why is the new threshold not used? How would it influence the comparison to ECMWF data?

The new threshold needs further justification before it can be considered in future studies. I can understand the idea behind the new pathways from NAT to ICE and from STS to ICE. However, the threshold itself is not well-founded. I can also see the threshold starting at 0.4 and/or may even be a tilted line starting at 0.45 1/R and decreasing with higher depolarization. Why is the threshold the same for both pathways? It could be different? More observation from the airborne lidar should be used to derive a better statistical justification of the findings. CALIPSO observations of the same clouds should be used to clarify and test the new threshold. What is the influence of instrumental differences between CALIOP and WALES on the classification? These issues require further discussion. Achtert and Tesche (2014) present an application of the CALIPSO classification scheme to ground-based lidar measurements and discuss that thresholds need to be adapted for meaningful classification. They also present the effects of measurement and instrumental errors on the classification. The present paper would benefit from a similarly thoroughly treatment of the data.

Specific points: - The duration and the maximum extension of ice PSC (and NAT + STS mixtures) are shown in Figure 1 and discussed in Section 3. From the text it is not clear what threshold was used for the classification of ice PCS. From the layout of the paper I assume it is still the old one. If so, the calculations need to be repeated with the new threshold. Currently the areal comparison to ECMWF is quite good. I expect that the area derived from CALIPSO observations will be larger with the new threshold. How can you explain the difference? How does the duration, extension and area compare to previous years when the new threshold is used? Again, further statistical analysis of the long CALIPSO time series in needed to justify changes in the choice of threshold values.

- Page 6, line 143: Is the linear particle depolarization ratio from the CALIPSO measurements derived the same way? How reliable is the CALIPSO depolarization value? Are the values and accuracy of the linear particle depolarization from WALES comparable to CALIPSO?

- Page 7, line 160: The new threshold seems to be unjustified. The only justification given is the form of the histogram. Why did you pick 0.3 and not 0.4 or 0.25? More discussion is needed.

- Page 7, line 162: The form of the histogram is quite different to previous published observation from the CALIPSO PSC team. How do you explain the difference? Is it just the case for the shown case study? What does it look like for the other WALES observations?

- Page 7, line 167: The comparison of the PSC classification and temperature generally agree. But one case is not enough to convince the reader of anything! It could be just coincidence. More observations/better statistics are needed to proof the quality of the comparison. Why do you not use the other observation of ice PSCs from WALES measurements?

- Page 7, line 170: What is the lidar ratio of NAT? How do you know the NAT lidar ratio? Please provide references. I am only aware of the paper by Reichardt et al. (2004) who state that the lidar ratios of solid and liquid PSC particles are similar.

- Page 8, line 188: Your typical NAT trajectory was never well below T-ice. Does NAT not form well below T-ice (3 to 4K below Tice)? It can however exist at warmer temperatures (up to T-nat). Was the NAT formed in the last 12 h within the ice layer or before -240 hours?

- Page 9, line 201 and page 10, second paragraph: How was the NAT formed if the temperature was never below T-ice?

- Page 9, line 202: Can you please provide the reader with CALIPSO observations? I am sure there are observations along the back trajectories that coincide in time. These observations can be used to support your discussion. Further, it would be good to use a box model along the back trajectories to understand the formation of ice and NAT particles better. One example on how to use back trajectories to investigate the

formation and alteration of a PSC observed by CALIPSO and a second lidar can be found in Achtert et al. (2011).

- Figure 4A: Please add markers for every 12h (or 24h).

Achtert, P., and M. Tesche. "Assessing lidar-based classification schemes for polar stratospheric clouds based on 16 years of measurements at Esrange, Sweden." Journal of Geophysical Research: Atmospheres 119.3 (2014): 1386-1405.

Reichardt, J., et al. "Mountain wave PSC dynamics and microphysics from ground-based lidar measurements and meteorological modeling." Atmospheric Chemistry and Physics 4.4 (2004): 1149-1165.

Achtert, P., et al. "Investigation of polar stratospheric clouds in January 2008 by means of ground -based and spaceborne lidar measurements and microphysical box model simulations." Journal of Geophysical Research: Atmospheres 116.D7 (2011).

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